

it meant a permanent diamagnetic polarity, that is to say, "*a diamagnet*," which seemed an incomprehensible result. My suspicions were at once aroused, however, as to the possibility of a transverse ordinary magnetisation; and subsequent experience, on the whole, confirms this explanation.

All the substances were then tried over again for permanent magnetic polarity, and not one of them has failed to show it. A piece of wood, for instance (or any other substance), which points axially between the poles, instantly reverses its position, turning through 180° , when the magnet is reversed. But the reversal must be done with the weak current only: anything like a strong current, *e.g.* that from two or three secondary cells, instantly destroys and reverses the permanent magnetism, and no abnormal behaviour is then detected. Some substances, however, retain it better than others. The permanent magnetism requires a strong current to excite it, and a very weak reverse current to detect it. Without these conditions it would certainly have been overlooked. It does not seem to matter whether a substance is magnetic or diamagnetic, it always reverses its position or nearly reverses it when the weak reverse current is applied.

The piece of copper was next held long-ways in the field, and a strong current applied. On now hanging it at 45° in the field, and testing it by a weak current, it at once returned to its axial position (though the copper was electrolytically "pure" and decidedly diamagnetic); on reversing the weak current, it at once turned through 180° , setting axially the other way, thus behaving exactly like a weak magnet. When a strong current was applied this behaviour was lost, the piece set itself nearly equatorially again, and the residual axial magnetism was either masked or lost. The reaction of induced currents naturally makes the examination of conducting masses rather troublesome.

During Christmas week Sir Wm. Thomson happened to pay the laboratory a hurried visit, and I showed him a piece of pitch-pine between the poles behaving exactly like a weak compass-needle; "a wooden magnet," as he at once called it. He was good enough to suggest a better mode of arranging the experiment for my original purpose of looking for the conversion of magnetism into diamagnetism: an arrangement which I have since adopted. So far, however, the results in this direction are very preliminary.

In all these experiments there is one flaw; and it is partly owing to this flaw that I have regarded them as unfit for publication. Indeed, I only send this note now because of the publication of Dr. Tumlirz's result. His experiment with quartz is very like one of mine, and it is very clearly and neatly described in his paper. But the same flaw, or what appeared to me to be such, seems to extend to his case also. What guarantee is there that no trace of iron is present,—perhaps as mere dirt, more likely as an infinitesimal ingredient of the substance? Several of the pieces of coke I used had been boiled for weeks in several lots of hydrochloric acid, and the last few washings gave no ferrocyanide coloration; but I have no doubt the coke yet contains iron. Possibly the other substances do too.

Suppose, now, a substance contains a trace of iron, which iron is susceptible of permanent magnetism, no matter how feeble: then, no matter whether the substance itself be paramagnetic or diamagnetic, in an intense field its own properties will altogether overpower those of the trace of iron, for this trace may be considered as magnetically saturated and done for at once. But suppose the substance next finds itself in a very weak field: the induced magnetism and the force depending on it, since they vary as the square of the field, are vanishing quantities; any trace of residual or permanent magnetism has it all its own way.

What way is there of proving that not a trace of iron exists in a body? Chemical tests are surely futile compared with the test of a magnet. I see at present no way out of the difficulty.

And would not the same difficulty recur in connection with my original notion? I believe it would. Suppose I succeeded in finding a substance, paramagnetic in a weak field, diamagnetic in a strong one; it would be open to any one to object that the paramagnetism was due to a trace of magnetic impurity: that this impurity, being intrinsically highly susceptible, causes all the observed action in a weak field, but that it soon becomes saturated as the field increases in strength, and that then its force is altogether overpowered by the main bulk of less susceptible substance, whose saturation-point, if existent at all, is miles higher up. This substance then regulates the behaviour of the body, and, according as it is diamagnetic or magnetic, the whole body behaves diamagnetically or magnetically.

Notwithstanding the prevision of this difficulty, I determined to try the experiments, not knowing what might come of them, and thinking that a body which could be made magnetic or diamagnetic at pleasure would be of some interest, however its behaviour might be explained. I even thought of artificially constructing such a body by incorporating a trace of iron in a lump of bismuth, or by using semi-purified commercial bismuth. I have not done this yet, however, and accordingly do not know if it be possible.

That which has come, so far, of these experiments, viz. the apparent existence of magnetic retentivity in all matter, is in itself not an improbable result; rather, one might say it is a probable one; and although it may be possible to explain it by a trace of iron impurity, it by no means follows that this disappointing sort of explanation is the correct one.

The singular fact which most strongly suggests the need for some such explanation is that diamagnetic bodies are capable of ordinary permanent magnetism. It is true that on the Weber-Ampère theory the specific molecular current of a diamagnetic substance need not be zero but may have a small positive value, which is easily destroyed and reversed by a powerful field, but which yet may endow the substance with magnetic properties in a weak field. But the worst of it is that I have never been able to detect any trace of paramagnetic property, in a diamagnetic substance, other than this permanent or residual polarity excited by immersion in a strong field.

The only suggestion I can make is the following.

Let the molecular channels in a diamagnetic substance be not wholly free from resistance, though their resistance must be very small; let induced currents be excited in these channels by immersion in a magnetic field, and let them have time to dissipate a little energy and begin to die away before the field is removed. On now destroying the field, the inverse induction will more than destroy the previously induced currents, and will leave a residue of opposite current in the molecules; the body will therefore behave as a weak magnet, until these residual currents die away.

I must examine more carefully the excitation and decay of the permanent magnetism with time. With wood it seems to be a question of hours.

These dissipation experiments are very important and should be seriously attempted in several directions with proper appliances and funds. Thus: gas molecules appear to be perfectly elastic, or rather their impact coefficient of restitution is supposed to be unity, but if a box of gas be shut up in infinitely adiabatic cotton-wool for a century, will it have gone colder?

Again, iron molecules are supposed to be infinitely conducting, and their Ampère currents seem permanent; but if the moment of a bar were measured from time to time against gravity, when a given current circulated in a given helix round it, would it be found that age impaired its strength?

Once more, bismuth is supposed to be diamagnetic by reason of its non-resisting channels; but suppose a piece of bismuth is left in a constant magnetic field for a year, will it have lost some of its diamagnetic property? and when taken out will it be found magnetic for a time?

It may be remarked that, whereas it is certain (on Ampère's theory) that iron molecules are almost infinitely conducting, we have no similar assurance for bismuth; it is even possible to surmise that a body may tend to become diamagnetic in proportion as it chokes off its own molecular currents, while magnetic bodies are such as retain them perennially without apparent loss. If such were the case, diamagnetism would sometimes improve with age.

I am, of course, aware that there is another, and merely differential, theory of diamagnetism, but this leaves magnetism itself wholly explained; whereas, directly the Ampère theory of magnetism is even provisionally accepted, the truth of infinite molecular conductivity is already crossed, and the Weber theory of diamagnetism follows as a natural and indeed inevitable consequence.

OLIVER LODGE

University College, Liverpool, March 15

Dissociation and Contact-Action

IN a recent issue (*NATURE*, vol. xxxiii. pp. 350-51) you drew attention in your "Chemical Notes" to some recent researches of M. Kononoff on "contact-actions," and to the suggestion made by him that "the dissociation (in the cases referred to) was a consequence of the contact-action of the solid body." On

referring to the abstract of M. Konovoloff's paper in the *Journal* of the Chemical Society for January, 1886, I meet with the following:—"As an explanation of this contact-action phenomenon it is asked whether it is not possible that the bombardment of the molecules on the solid matter causes the kinetic energy of the molecules to be transformed in part into the internal work required for their decomposition."

Perhaps some of your correspondents will kindly furnish me with references to original memoirs (or other sources of information) in which I may find this question competently treated. The idea here put forward by M. Konovoloff is surely not new. It might be extended, as I conceive, to such cases as, for example, the combination of SO_2 and O_2 to form SO_3 , the formation of ammonia from a mixture of NO and H_2 in the proportion of $5\text{H}_2 + 2\text{NO}$, the formation of NO from a mixture of NH_3 with an excess of O_2 , in each case when the gaseous mixture is passed over heated platinum-sponge or platinised asbestos. For some years past this explanation of such contact-action phenomena has appeared to me much more reasonable than such explanations as are generally suggested. The high temperature required in such cases seems to point rather to something in common with the initial dissociation caused by the intense heat of the electric spark, when oxy-hydrogen gas is fired. In such cases as those referred to above the lesser intensity of the heat applied from without may easily be compensated by intra-molecular results of the increased energy with which the impact of individual molecules must take place at high temperatures, and the great extension of the heated solid surface exposed to their bombardment. Under this view (with which my pupils have been familiar for some years past) combination is brought about through the atoms of some of the molecules of the mixed gases being brought into the *quasi-nascent state*.

A. IRVING

Wellington College, March 10

Variable Stars

IN NATURE for March 11 (p. 440) Dr. Mills, in criticising Prof. Seeliger's *collision hypothesis* of the blazing forth of *Nova*, advances a theory of his own as a presumably original and novel explanation of the phenomena of variable stars. It may be of interest, therefore, to point out that practically the same explanation was suggested in 1878 by Prof. R. Meldola in a paper published in the *Philosophical Magazine* for July of that year.

In this paper the author states: "It is conceivable that in certain cases the composition of a star's atmosphere may be such as to permit a considerable amount of cooling before any combination takes place among its constituents; under such circumstances a sudden catastrophe might mark the period of combination, and a star of feeble light would blaze forth suddenly, as occurred in 1866 to τ Coronæ Borealis. In other cases, again, it is possible that the composition of a star's atmosphere may be of such a nature as to lead to a state of periodically unstable chemical equilibrium; that is to say, during a certain period combination may be going on with the accompanying evolution of heat, till at length dissociation again begins to take place. In this manner the phenomena of many variable stars may perhaps be accounted for."

It will be seen that these hypotheses are essentially identical, although it would appear that Dr. Mills limits his explanation to the formation of polymerides (presumably of some primordial matter), these constituting our chemical "elements." I cannot see, however, that he has any reasons for excluding the formation of true compounds, or why he should consider a variable star as necessarily one that is engaged *only* in "making elements." This last process would, no doubt, be the first to take place on the hypothesis of cooling from a state of complete dissociation, but there would surely come a period when the more stable chemical "compounds" could exist, and their formation would also be attended by the evolution of heat and possibly of light also.

JNO. CASTELL-EVANS

London, March 13

The Iridescent Clouds and their Height

COL. TENNANT is mistaken in supposing that the only peculiarity of the clouds which appeared in December 1884 and 1885 is in their being fringed with coloured spectra, though these were, I believe, much more vivid than those of ordinary clouds, as described by him; besides which, my impression was that the colours were more varied than is usually the case. Col.

Tennant, with his experience, will be better able to say than I am whether there is generally as much blue in the clouds he describes as in those under discussion. I stated in my letter of Dec. 29 last (p. 199) that there was no special amount of blue in the clouds seen the previous day, but on the 31st there was a good deal. However, I do not insist on this as being any important difference; but, by referring to the numerous letters this year and last about the clouds, he will see there were several other characteristic points.

These clouds are not like any ordinary clouds; if they can be referred to any of the usual classes they are cirrus, but decidedly different from any cirrus we generally see. Their usually very smooth texture was striking, though some on December 28 (1885) had the ordinary appearance of rippling, but in most cases this was too slight to be visible without optical aid, even when the clouds were broken up into narrow wisps, and in such a position that no colour was produced there was still something in their appearance which struck me as different from ordinary clouds. The frequently rectangular shape was very singular also, though they had not always this form. I said I had not observed this shape in the clouds of December 28, but other observers noted it on that occasion (see pp. 219, 220), and on the 31st I saw many of the clouds with this outline. It is shown indistinctly and with the corners cut off in Mr. C. Davison's sketches (pp. 292, 293). The form is generally described in the letters you have published as rhomboidal, but this is an effect of perspective; no doubt if the clouds were seen overhead they would appear rectangular. Their great height, too, must have been unusual, though perhaps not greater than that of the singular coloured clouds seen last summer in Bavaria by myself and in this country by others, as described in NATURE, and which differed from the clouds I am now describing in some important particulars. One patch of cloud was observed both here and at Shields on December 28, and a calculation from a comparison of the position as seen from the two places gives its height as 23 miles; while making the utmost allowance that seems permitted for the roughness of the observations only reduces its elevation to 11 miles. That it was the same patch of cloud observed from both places is undoubted, for one observer of it (H. R. Procter) was travelling from Shields to Sunderland, and he saw that it was the same patch all the time, and the one I had been observing here. The fringes of colour were distinctly visible on this cloud up to 4h. 25m., and feebly so till 4h. 27½m. I concluded that the sun had not ceased shining upon it till that time; if so, its height would be between 11 and 12 miles. At 4h. 28½m. it was pink with sunset colouring; but the sun need not have been shining on it then.

The iridescent colours have no connection with halos, as supposed by Mr. Stone (p. 391), no particular colour appearing at any particular distance from the sun, but every colour being seen at any distance, though more vividly at perhaps from 15° to 30° off the sun.

THOS. W. BACKHOUSE

Sunderland, March 12

Forms of Ice

A CURIOUS formation has lately occurred on the surface of a sheet of ice in a tub. Being under a tap, the ice became submerged below several inches of water. Fresh ice then formed as thin vertical plates upon, and at right angles to the submerged sheet. These plates meeting each other in all directions, produced a spongy mass, 3 or 4 inches thick. I do not know if it is a common production, but the special interest attached to it is that it would seem to suggest how "spongy" quartz has arisen, of which I have a specimen consisting of thin and nearly parallel plates; as well as the well-known form of thin crystalline plates in which calcite may occur. It is just this form of calcite which gives rise to "hacked" quartz, when silica has coagulated or crystallised over a mass of such thin crystals, and then these latter have been subsequently dissolved out.

Why a sheet of ice should increase regularly in thickness by additions to its lower surface, and form this spongy mass on its upper, is a question I should like to hear solved.

Another form of ice I lately noticed on a wall consisted of minute prisms standing in little depressions in the bricks. The circumference of the prism partook of the irregular form of the cavity, giving the appearance of an upward growth.

While speaking of ice, I should like to venture a suggestion to account for its lighter specific gravity than that of water, namely, that water crystallises in masses of complex form; the